PROCEEDINGS OF SCIENCE

Discussion on initial conditions, minijet survival, quadrupole vs minijet systematics

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This is a transcript of the discussion which followed the second day of talks at the workshop. The participants were: Rene Bellwied, Helen Caines, Yuri Dokshitzer, Rainer Fries, Ulrich Heinz, Jiangyong Jia, David Kettler, Christina Markert, Denes Molnar, Guy Moore, Lanny Ray, Thorsten Renk, Raimond Snellings, Mike Tannenbaum, Derek Teaney, Dylan Thein, and Tom Trainor.

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1. Introduction

The discussion on the second day of the workshop focused on hydrodynamics, the thermalization assumption, a critical evaluation of the data most relevant to the hydrodynamic paradigm, the minimum-bias jet interpretation, the implication of the hydrodynamic interpretation for semi-hard parton scattering and jets, the implication of observed correlation structures for the hydrodynamic paradigm, and the data which falsify, or which may in the future enable one or both paradigms to be falsified. The audio recordings were transcribed as closely as possible to the actual statements without editing. Additions inserted during the editing process to clarify the speaker's intent are enclosed in square brackets. Additional, contributed comments made after the workshop are identified as "Note added." Unintelligible audio portions are indicated by "[???]."

2. Possible discussion topics

Lanny: Early thermalization, less than 1 fm/c. To me this is a huge problem. We had a lot of good talks on hydro, but one problem not discussed is HBT [in relation to hydro predictions]. What does it take to falsify hydro? [Mike: Too late, it cannot be falsified... laughter] There are technical issues with estimating initial-state eccentricity. There's a lot of dispute about that. We focused on v_2 as a function of particle mass, especially at low p_t. Uli has made a claim that [mass ordering] is evidence for thermalization. Our analysis in STAR [Estruct group] seems to indicate that [such mass ordering] results from a uniform [single-value] transverse boost. Viscosity inference seems to depend on $v_2(p_t)$ trends. What we've shown in more-central data is that v_2 is dropping, much more than these [hydro] models have addressed [predicted]. Does that mean viscosity runs up through the roof? What does that imply? Some results that Duncan and I showed from fitting 2D angular autocorrelations: are those stable, reliable, unique? Is the same-side peak structure in 2D angular correlations [caused by] some other mechanism (e.g. flow mechanism), not jets? Is there a way to definitively decide among proposed mechanisms? There is an emerging idea of opaque hydrodynamic core with hadronic corona. You could have surface jets there, but we've shown the [centrality] systematics which argue against that.

Guy: How many hours do we have to discuss that? [laughter]

Tom: People should talk about what they want. This is a fallback.

3. David's p. 26

Thorsten: I would like to see the famous page 26 of the [David's] morning talk. I would like someone to step up and make a very provocative statement why this argues against hydro, because I didn't get the point.

[topic delayed until later]

4. What is the actual $\mathbf{v_2(p_t)}$ for 0-5% Au-Au collisions?

Yuri: While that is setting up let me ask an outsider's question. Why doesn't v_2 vanish for central collisions?

Thorsten: $b = 0$ exists in the theorist's mind, not in [reality].

Tom: In 0-5% [central collisions] v_2/ε from hydro should be quite significant [nonzero]. These observations [David's measurements] contradict that expectation by a lot.

Yuri: Because of fluctuations [in eccentricity]?

Tom: No, because that's where you are in centrality [b]. Let's forget fluctuations. 0-5% corresponds to a significant impact parameter. It's like 3.5 fm.

Raimond: I would not take as a fact that for 0-5% v_2 is zero. There's one estimate of v_2 which is zero.

Uli: There's lots of published data that are nonzero.

Raimond: That's right. And experimentally it's extremely hard to get a number there. So, maybe the most conservative thing to do would be to just draw a smooth curve from all the centralities through zero and then estimate what the number should be. I don't think any of our measurements would be able to do much better at it. It is measured with a large error bar at that centrality. That's my take on it.

Uli: I would say, somebody asked how you can falsify hydrodynamics. If you find that in the 5% most-central collisions there is really zero v_2 then I think we have a problem with hydrodynamics. Because those collisions have, just by fluctuations, they don't have perfectly round [zero eccentricity] initial conditions. So, if the system thermalizes it will have an anisotropic flow developing, and we will see the [nonzero] v_2. Any [hydro] calculation will predict this. And if you don't measure it, it's a problem. But, that's why I'm somewhat hesitant to believe that value that has been thrown out here today of around zero. Because I have seen so many other values that are already in the published literature that contradict this data.

Note added (Tom): What is being contrasted is two fundamentally different measurement techniques – a) nongraphical numerical methods denoted $v_2\{\text{method}\}$ applied to azimuth correlations (published) and b) model fits to 2D angular correlations on azimuth *and* pseudorapidity denoted $v_2\{2D\}$ (not yet published). Most of the methods a) produce very significant non-zero v_2 for 0-5% Au-Au; method b) is consistent with zero and establishes a small upper limit which strongly contradicts methods a). It is not justified a priori to describe a) and b) in terms of a common systematic uncertainty.

Derek: Why would you mess around with this 0-5% bin where you can't measure? You go just a tiny little bit away [to less-central bins] and then you have a beautiful measurement, and it's large. And it's the right size predicted by hydro.

Mike: I have another way to falsify hydro. Uli predicts at LHC v_2/ε goes up 15%. Wit Busza predicts it goes up 60%. If Uli is right hydro is not falsified. If Wit is right, I assume it's falsified. Is that true?

Uli: If they confirm elliptic flow [v_2] that's a factor two bigger at LHC than at RHIC then [Derek: it's really tough.] Yes.

Derek: There's one caveat here, which is [the possibility that] eccentricity changes with energy.

Uli: But a factor of two is hard.

Raimond: But Wit's [prediction] is not a factor two, it's [increase, decrease?] 60%.

Guy: If it goes down again that's also important.

Tom: Back to Uli's comment. Here's [p. 22, David's talk, right panel] the 0-5% centrality. The open circles are the published event-plane [EP] data [one of the methods a)]. The lower solid curve is the upper bound from the 2D measurement [method b)]. As David pointed out, the dash-dotted curve is $v_2(p_t)$ inferred *only* from the same-side 2D jet peak, which includes the [peak] width variations. In other words, the Fourier amplitude from the jet peak depends both on the azimuth width—the narrower the azimuth width the larger will be the Fourier amplitude—and also on the η width, because that determines the projection of that [2D] peak onto the azimuth axis. For instance, you see the dotted curve. That's the same exercise if you don't allow the widths to vary. Maintaining the widths at fixed values you get too high v_2 values for the largest p_t values. The dash-dotted curve comes down at larger p_t because the η width of the same-side jet peak narrows again. At 5-6 GeV/c it has returned to the p-p value. That explains the trend of $v_2(p_t)$ [for 0-5% central collisions and method a)]. It is the η width of the same-side jet peak [that determines the variation of v_2]. At 6 GeV/c the same-side 2D peak is no longer broad on η. It is unmistakably a symmetric jet cone.

Note added (Tom): That panel demonstrates that to within a few percent of the published EP data the event-plane v_2 values are determined entirely by the same-side 2D peak structure, whatever mechanism is attributed to it, not by the (nonjet) quadrupole denoted by $\langle\cos(2\phi)\rangle$ with respect to the reaction plane expected in a hydro context.

Uli: But that doesn't address the question whether hydro is wrong or not. Let's forget about 6 GeV/c particles when we talk about hydro.

Note added (Tom): The demonstration on p. 22 relates to whether published v_2 data at any p_t correspond to a nonjet quadrupole, whatever its interpretation, or to the same-side 2D peak which can be interpreted in terms of jets. If the latter, then a hydro interpretation is very doubtful.

Rene: Didn't you show that $v_2\{2D\}$ [from fits to 2D angular correlations] is in agreement with the four-particle cumulant [$v_2\{4\}$]?

Lanny: We don't have $v_2\{4\}$ for this [centrality] bin.

David: That's a good point. If you are talking about the most-central bin, Uli said there was lots of data out there, but there is actually not very much data out there for the most central bin. In fact I don't know of any data for the multiparticle methods. They're all two-particle data.

Rene: Well, not as a function of p_t but as a function of centrality it's there. You want to really look at where hydro is applicable [on p_t]. And that's a small part of your spectrum.

Tom: Well, certainly down at 1 GeV/c there's the same problem. You [David] should show the comprehensive [$v_2(p_t)$] survey. That's very important.

David: [p. 15, left panel] $v_2\{4\}$ is the open squares. $v_2\{2D\}$ is the solid points. There is no [p_t-integral] $v_2\{4\}$ for 0-5% central.

Tom: You need to go to the comprehensive p_t-dependent, centrality-dependent [2D] data.

Rene: So, there's one point missing for $v_2\{4\}$?

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Raimond: That's correct, because $v_2\{4\}$ cannot be measured at that point, because the correlation is too small. They don't have a reliable estimate.

Note added (Tom): That would seem to corroborate David's $v_2\{2D\} = 0$ result for 0-5% central Au-Au. The 2D measurement at 0-5% is no more difficult (at $v_2 \sim 0.01$ level) than at other centralities.

Tom: This is a much more comprehensive representation [p. 31 of David's talk]. The solid dots are the [2D] 200 GeV p_t dependence. What you saw on the previous slide [p. 22, right panel] was the bottom-right panel of the survey. What's also included here, the two bold dotted curves, are parametrizations of $v_2\{4\}$ (lower) and $v_2\{FTPC\}$ (upper) data. Note the labels in the upper-right panel. At 0-5% the $v_2\{4\}$ *parametrization* [used in STAR triggered dihadron jet correlation analysis] is very nonzero, and $v_2\{2D\}$ is consistent with zero [upper limit small compared to published data]. There's a large discrepancy.

Uli: OK, this is where the measurement is hard. Let's take an intermediate impact parameter where the signal is bigger and the measurement is easier.

Mike: We have a preprint. It's 1003.5586 which clearly shows v_2 not zero in 5%, at $p_t = 3$ GeV/c, using the ZDCs. There's no autocorrelations, no nothing.

Note added (Lanny): "Autocorrelations" here refers to self pairs, a different usage of this term than in our presentations where we mean the correlation of a distribution with itself.

Rene: That's a two-particle measurement.

Mike: It's not, its reaction plane—one particle.

Note added (Tom): The reference is to the event-plane method conventionally denoted EP. To a few percent $v_2\{EP\} \approx v_2\{2\}$ in some STAR v_2 papers.

Rene: It's what you would call $v_2\{2\}$.

Mike: I would call $v_2\{2\}$ two-particle. We do that too. We don't have one particle there. We have *all* the particles. [Tom: But you're taking them in pairs.] We're not taking them in pairs. We calculate event-by-event the event plane, like you guys do, except there's no autocorrelations, because it's [ZDC at large η].

Note added (Mike): That is, because the reaction plane detector is in a totally disjoint region of phase space and we use four different reaction-plane detectors, as discussed in detail in that preprint. In Fig. 2 we show the result (actually the ratios) with the many different reaction-plane detectors.

Note added (Tom): The event-plane $v_2\{\text{EP}\}$ and two-particle $v_2\{2\}$ methods are algebraically equivalent (after event-ensemble averaging) to within a few percent when measured within the STAR TPC acceptance. See 0803.4002 for a detailed algebraic comparison of several relevant v_2 methods

Rene: There's no error bars on those points [from 1003.5586].

Mike: Why are four explicit particles better than one with a hundred?

Rene: I also think if you look at these small numbers in the most central bins you need some error bars. You need some systematic error bars.

Tom: Systematic error bars [uncertainties] are not going to exclude [accommodate] at least a factor 30 disagreement in these results [David, p. 22].

Uli: I suggest that experimentalists sort this out before I get involved in it. [laughter]

Mike: You may have to wait a long time, because it's religion, it's not science.

Uli: Until then I go by what's published.

Rene: That's not quite fair. You just told us we can only falsify your theory if we look at the most extreme [centrality] bin. And now we're looking at the most extreme bin and you say "Ah, I don't want to look at the most extreme bin."

Uli: I'm not worried about it.

5. David's p. 26 continued—Inferring a boost distribution

Thorsten: Could we go back to p. 26 [David's talk] and someone explain to me where the...

Tom: Thorsten, do you want to ask a specific question or do you want this explained?

Thorsten: I want this explained. Why does hydro have a problem with this?

Tom: OK, here's my interpretation of the middle panel. Rapidity calculated with proper mass [for each hadron species] is a velocity measure. You are calculating basically the log of p_t/m_0. In fact, in the low-p_t limit that [y_t] is just p_t/m_0. So, we have these spectra plotted in this way. The [choice] v_2/p_t is simply because v_2 is approximately proportional to p_t anyway [see 0803.4002], so we take that factor out and see what is left. When we plot v_2/p_t on a speed measure we are really looking at the boost. The hypothesis is that these hadrons are coming from a moving source.

Uli: There's thermal smearing, right? The velocity of the particle is not the velocity of the cell, because there is a thermal momentum distribution around that.

Note added (Tom): The extended particle spectrum relative to boost apparent in the middle figure is a variant of the "thermal smearing" expected by Uli.

Tom: This is purely Cooper-Frye: I have a moving source, I convert to a hadron spectrum, I observe the hadrons [in the lab]. This is where [why] you would expect the mass dependence for instance. So, we want to work backwards through the $v_2(p_t)$ data and ask what is the boost distribution [of the particle source]. That [boost distribution] is what hydro is predicting. So, you do your hydro thing. You produce a velocity profile or distribution...

Thorsten: Essentially you are trying to get to the flow profile [from $v_2(p_t)$ data].

Tom: Exactly. So, this thing [middle panel trends] can be expressed as, in Cooper-Frye language, the folding of a boost distribution with the hadron production – a [conditional] spectrum depending on boost. In mathematical language the conditional spectrum is the kernel of the integral equation. Inserting that kernel as a hypothesis (e.g. assume Maxwell-Boltzmann) I should be able to invert the integral equation to infer the source boost distribution. I can do that by eye here.

Uli: No, no. I want you to do that as you said. Then I want to see that distribution with its width and the error bars. You're stating that you can do that by eye and you find a delta function. And I don't believe that.

Tom: This can be done numerically. But, from these data v_2 is the ratio of a spectrum in the numerator, and one in the denominator. The spectrum in the denominator is simply the single-particle p_t spectrum, which we know. What's unknown is the spectrum in the numerator [denoted the quadrupole spectrum]. As David has implicitly shown in another slide, the ratio of these two spectra is essentially $\exp(-p_t/4)$ [pp. 29,30]. That's it. So, $v_2(p_t)$ is $\propto p_t \exp(-p_t/4)$ for 200 GeV Au-Au. That describes all the $v_2\{2D\}$ data to within their errors, which are very small.

Uli: I didn't understand what you said. Can you write this down what you are saying? What is the spectrum in the numerator that's different from the denominator?

Tom: That's the quadrupole spectrum. David showed you [p. 25] the quadrupole spectrum inferred from his measurements. So, $v_2\{2D\}(p_t) \propto p_t \exp(-p_t/4)$. It's also got [as factors] $\langle 1/p_t \rangle$, which is about 2.1 in appropriate units. And it's got $v_2\{2D\}(b)$, the p_t-integrated centrality dependence [of v_2]. And that's it. This describes the p_t dependence of the data. In summary

$$v_2\{2D\}(p_t, b)/p_t \approx \langle 1/p_t \rangle v_2\{2D\}(b) \exp(-p_t/4). \tag{5.1}$$

That describes the data comprehensively for all centralities and all p_t [up to 6 GeV/c] at 200 GeV.

Mike: What do you learn from that?

Tom: Life is very simple.

Uli: But that goes through zero at $p_t = 0$.

Tom: That's on p_t [and is for all hadrons, dominated by pions]. [go to p. 26]. For pions that's rapidity 0.6. The corresponding p_t is $0.14 \sinh(0.6) = 90$ MeV/c. You can't see that on a [typical] p_t plot.

Rene: Where does the black line come from?

Tom: That's the [boosted] quadrupole spectrum [p. 25] divided by the single-particle spectrum.

Rene: It's boosted by the common boost parameter? Where does that common boost come from?

Tom: It's inferred from the data.

Rene: That's very circular argument.

Note added (Tom): The *value* of the common boost is inferred from the data. The *mechanism* for the common boost is the subject of speculation.

Uli: You can boost the single-particle momenta, right?

Tom: Here's the plot you saw, with protons, kaons and pions [p. 25].

Uli: What is the horizontal axis?

Tom: y_t with the appropriate mass, the true rapidity. These data require this offset, especially the protons. These are simple Levy distributions shifted over to the rest frame of the boosted system. They all go to the same place [intercept at lower limit]. This is implicit in what you're doing [interpreting $v_2(p_t)$ in terms of hydro models].

Mike: What quantity do you learn from this? You collapse it all and therefore you learn something. What is the measure that you get out of this.

Tom: The boost distribution of the particle source.

Mike: And what does that mean. What is it and what does it mean?

Derek: Why are you dividing zero by zero always? Just like before, we have v_2 going to zero, we're dividing by zero. Here we have $v_2(p_t)$. It's going to zero, you're dividing by zero. We have v_2/p_t and it's going to zero.

Rene: The nice thing about p_t is that it starts at zero. This thing [y_t] doesn't start at zero. [laughter]

Note added (Tom): The limit of y_t for small p_t is just p_t/m_{hadron}. y_t does "start at zero."

Derek: Why are we doing that?

Tom: I want to know what can be compared to your [hydro] calculations. I want to know the boost distribution of the [particle] source. This is how you do that. This [plotting $v_2(p_t)$ vs p_t] tells you nothing. This is a junk way to present this [v_2 data]. You want to plot the data on a velocity variable.

Uli: I don't understand this. This is the velocity of the pion or proton or kaon. This is not the velocity of the fluid. So, why is the velocity of the particle any better than the rapidity or the p_t?

Note added (Tom): What is plotted is the distribution of velocities (or their logarithms in the form of rapidities) of hadrons of several masses in the lab frame. From the combination it may be possible to infer the boost distribution for a common source ("velocity of the fluid") *and* the particle spectra in the boost frame.

Tom: Because it [what is measured] is the velocity of the source plus the velocity of the particle relative to the source.

Uli: Plus or minus. There's thermal motion around it. There is a flow velocity which... Even if you have a shell and you have a fixed flow velocity you have a distribution of particle velocities.

Note added (Tom): If one assumes isotropic emission from a moving source on 1D ("plus or minus") the problem with the inferred boost distribution deviating from hydro (Hubble expansion) becomes even worse. The inferred mean boost moves to larger values.

Tom: Here's Romatschke [curve B, middle panel, p. 26]. From this I can infer roughly, let me plot, a source boost distribution [flat distribution starting at boost = zero and continuing to about $\Delta y_{t0} \approx 0.5$ where it falls off to zero]. And here is what is built into a typical hydro calculation, something like that [similar curve]. That's a consequence of radial flow proportional to radius. You integrate [a conditional source spectrum] over a source distribution on radius which produces hadrons. What we're inferring from the [$v_2(p_t)$] data is a source distribution which looks like that [narrow peak near 0.6].

Guy: Question, B is Romatschke's curve fitting the black pion data?

Tom: Yes. Not fitting, predicting.

Guy: Point to the last (smallest y_t) pion data point. And what's its error bar? So, it's 1-1/2 sigma from his curve?

Tom: It's the protons that kill the hydro calculation, not pions. This [p. 27] is plotting only the protons, and this is Romatschke's proton prediction [dash-dotted curve].

Rene: Can you show the proton spectrum on p_t? Are you measuring 90 MeV/c protons? I'm trying to relate the y_t which you have there, 0.8 to p_t.

Tom: That's [$y_t \sim 0.9$] roughly 1 GeV/c for protons, since it's mass times hyperbolic sine.

Note added (Tom): $p_t = m_{hadron} \sinh y_t$, for example $p_t = 0.94 \sinh(0.9) = 0.96$ GeV/c for protons.

Rene: So there are proton points that are way below that.

Tom: This is all that was in the referenced STAR v_2 paper.

Rene: Where are all these proton points below $y_t = 0.8$?

Tom: The [preliminary] data that have just come from the flow group in STAR do extend below this range [and have negative v_2 values there].

Rene: The lowest proton point in terms of p_t that we measure is 0.2 GeV/c [for spectra].

Tom: This is not cherry picking, this is what was available as published STAR v_2 data two years ago. I'd be perfectly happy to accept from you more sensitive proton data, but this is what we have. So, this curve [including the boost inference] is inferred from the published data. The black curve [p. 26, middle panel] is calculated as a boosted Levy distribution divided by an unboosted measured single-particle distribution.

Thorsten: In very simple terms hydro predicts a proton can be at rest [in the lab] because it's at the center [of the collision, zero radius] where there is no flow field [no radial flow], whereas you claim that the data show a proton cannot be at rest.

Tom: Yes, in the coarsest interpretation of this [various plots].

Derek: You can put down the Romatschke calculation. But if you do hydro with any sort of pion wind afterwards, which is one part of the cascade I actually believe, you will change those points predictions from those hydro curves. Those lowest bins, that's the only place where the [hadronic?] cascade actually matters.

Tom: What does that mean in terms of this comparison [pp. 26, 27]?

Uli: The slope from the Romatschke curve can be all over the place.

Tom: Then hydro is not falsifiable.

Rene: It is if you can prove that the curve doesn't go through zero-zero. All hydro curves would go to zero-zero on that plot.

Uli: Yes.

Rene: If you can prove that it doesn't go to zero-zero you're right.

Uli: Exactly, so that's what I said.

Tom: Suppose the data went like that [very small positive values at lower p_t below the "step" in v_2/p_t].

Rene: Then hydro is correct.

Uli: Then at least you don't know whether hydro is wrong.

Rene: If you can show that you really have this boost that you're talking about then you really have falsified hydro.

Tom: What's going to happen then – I'm going to modify this [boost] curve this way [peak near 0.6 and small tail down to zero boost, to accommodate the unobserved data].

Note added (Tom): The modification corresponds to unphysical particle emission spectra.

Uli: That's actually something that you get out from hydro. You get a distribution of velocities that has a peak....

Denes: It's clear that when the system is not thermal anymore you have only some parameter description unless you run a transport that's the best one you have, which nobody in this room has done for viscous hydro. So, for ideal hydro only, Uli has the nice plot in QGP volume 3, the protons don't quite work. In ideal hydro the protons [theory] were never really on the measurement. We know this, with the [???] equation of state, the first-order phase transition, they were at least somewhat closer than with the hadron-gas assumption. But now we also know that the first-order phase transition is not really quite the same as what lattice QCD will tell us. Fine. For viscous hydro we don't have any reliable identified-particle spectra. That particular proton calculation [Romatschke???] relies on a crazy assumption about rates in a hadron gas...

Tom: Are we throwing out Romatschke? Is that the consensus?

Uli: This calculation is not...he would be the first to say...you cannot

Denes: Even if you want to write hydro...hydro alone will not give you the final result. It would be hydro plus a freezeout description plus an assumption over the initial conditions. Only this particular package you could test.

Uli: That's why I went through all of this this morning.

Derek: To make one last point here. Those particles, those few protons down there, are just a negligible fraction of the total energy-momentum tensor. So, to make a prediction from hydro with bulk energy-momentum tensor is reasonably reliable. Then you go to this negligible fraction, and you're sort of asking for trouble.

Uli: Not the best strategy to falsify...

Tom: That's the peak of the [single-particle] proton distribution.

Rene: Protons don't peak at 1 GeV/c.

Tom: OK, so we don't test hydro with data.

Guy: On the plot to the left [p. 26] what is the hydro prediction for Lambdas?

Tom: The dotted curve A is Derek's curve [for pions]. Romatschke's curve B [for pions] is the other dotted curve. So, it looks like it almost agrees with the pion data [in the left panel]. But in the middle panel its only for $y_t < 1$ that you're testing hydro [boost distribution]. And that area is here [below 0.2 GeV/c for pions] in this plot [left panel, on p_t]. It's invisible [in that plotting format].

Rene: And you have one [pion] data point there. I don't understand your argument, because you're testing it better on a linear scale with p_t, hydro, than you're testing it on a y_t scale. Because on the y_t scale it's much more compressed, the hydro validity part. The hydro validity part on the y_t scale is from 0 to 1.

Note added (Tom): The opposite is true. The critical hydro comparison region is *expanded* on y_t.

Tom: No. Here's where you're testing hydro on this scale [$p_t < 0.2$ GeV/c], for pions, and that's where you're testing hydro on that scale [$y_t < 1$].

Rene: In a region where you have one data point? You're testing hydro all the way up to 2 GeV/c in p_t. So, why do you say you're testing it down there [$<$ 0.2 GeV/c] for the pions. I don't understand.

Tom: Because that's where the difference in the boost distributions is occurring [for pions].

Guy: But you only have one data point for pions there.

Tom: That's the point. That's not my fault. For light mesons you don't get much of a test from published data. For protons/Lambdas all this interval [up to 2 GeV/c] is testing hydro.

Thorsten: You are sensitive to very slow hadrons. You want a pion or proton essentially at rest. You say that would test hydro. You are running into complications like quantum correlations, HBT, at some point.

Note added (Tom): HBT is excluded from the quadrupole contribution to 2D angular correlations at a level below $v_2 \sim 0.01$ in more-central Au-Au collisions.

Tom: I'm puzzled why this is so difficult to assimilate, because this is just Cooper-Frye.

Guy: Suppose QCD were in the chiral limit. Then the pion would be massless. Then you're saying there are no pions which would tell you anything about hydrodynamics.

Tom: I have no idea how to deal with a massless pion.

Guy: But you're close to that limit. The pion is very light. It's lighter than the temperature at chemical freezeout. Suppose we're almost in the chiral limit and the pion mass is 4 MeV/c^2. Then you're saying zero of the phase space of pions would be of any interest to hydro. That seems very hard to swallow. You're claiming that a tiny minority of the phase space of the pions is of any interest to hydrodynamics, which sounds crazy.

Note added (Tom): The lack of importance of pions to hydro tests is based on the low-p_t side of the boosted spectra. On the high-p_t side (as in CMB measurements) an apparent blue shift of the (massless) pion spectrum could result from a source boost or an increased source temperature. There are at least two issues in play: a) The amplitude of a particular spectrum or correlation structure attributed to hydro by hypothesis, which may test the predicted strength of a hydro phenomenon and b) the detailed structure of a predicted fluid boost distribution, which can be tested only by certain aspects of correlation structure. It is the latter which is questioned in this exchange.

Derek: Never mind that all those black points make up most of the stress tensor. And you're saying that's irrelevant for hydro?

Note added (Tom): See kinematics in 0803.4002 Eq. (14) and related discussion. For a particle source with Maxwell-Boltzmann (M-B) spectrum, fixed boost Δy_{t0} and particle mass m_0, p_t in the lab frame is $p_t = m_0 \sinh\{y'_t + \Delta y_{t0}\} = p'_t \cosh(\Delta y_{t0}) + m'_t \sinh(\Delta y_{t0})\} = \gamma_t \{p'_t + \beta_t m'_t\}$, where $p'_t = m_0 \sinh(y'_t)$ is p_t in the boost frame. $p'_t = 0$ in the boost frame (left edge of M-B spectrum) occurs at offset $p_{t0} = \gamma_t \beta_t m_0 = m_0 \sinh(\Delta y_{t0})$ in the lab frame. For $p'_t \gg m_0$ $p_t = \gamma_t(1+\beta_t)p'_t$, which is also the blue shift expected for massless particles (photons). A source boost therefore may have two manifestations: a) The spectrum at larger energy/momentum (the right "edge") is blue-shifted to larger momentum, but that blue shift, observed over a limited p_t range, may be confused with an elevated local temperature. That apparent blue shift is what COBE/WMAP CMB analysis relies on. b) For massive particles the left edge of the spectrum is shifted to a non-zero value in the lab frame. That effect is not present for photons and is not part of the CMB analysis. It is unique to the spectra of massive hadrons. [i.e., not pions] In the limit of small hadron mass the shift of the left edge is not accessible experimentally (due to limited p_t acceptance). Pions provide very marginal sensitivity to source boosts expected for hydro expansion, which is the subject of present discussion.

Note added (Uli): No transverse boost by a collective flow velocity, of any magnitude, is able to cause an edge of the p_T-spectrum at non-zero p_T as you (Tom) claim in point b) above. Due to the exponential tails of the (Maxwell-Boltzmann) momentum distibution in the boosted (or local rest) frame, the p_T-spectrum in the lab frame always remains non-zero at $p_T = 0$. This is true for particles of any mass.

Christina: Let me understand the picture. Say you have a boost. You have an initial boost that boosts every particle and in your picture no particle would interact.

Tom: I have no idea about final-state interactions. [Tom: We assume no parton or hadron rescattering unless proven otherwise.]

Christina: Where does the boost come from? How is it different compared to initial pressure.

Tom: This is a moving hot stove which is emitting black-body radiation. The resulting spectrum is blue shifted. [Tom: The mechanism for the boost is a separate issue which is a subject for speculation.]

Christina. And how does it affect the particles?

Uli: But you don't blue shift v_2 which is a deformation on the spectrum. You blue shift the spectrum itself. Looking at this v_2 plot and then shifting that around on a velocity scale just doesn't make any physics sense. This is not the way to extract any velocities.

Tom: The simple description is that there is an azimuthally-dependent boost. The elliptic boost is superposed on a radial boost. So you have $\Delta y_t(\phi) = \Delta y_{t0} + \Delta y_{t2} \cos 2\phi$ [with respect to reaction plane]. Δy_{t2} is a factor in v_2.

Uli: That's a nonsense formula. Sorry, what are you doing here?

Tom: I'm making a model of [azimuth-dependent] radial boost.

Uli: I can tell you how you do that. You write it in the Boltzmann factor e to the minus local energy, and this is the boost velocity.

Tom: That's exactly what this is.

Uli: It doesn't show up this way. I'm sorry. You take this distribution, you give it some phi dependence, give it a quadrupole contribution, calculate cosine 2 phi and integrate over whatever. And it doesn't give you this.

Tom: That's exactly what I did [following the Cooper-Frye formalism]. I put this [azimuth-dependent boost] into your formula, and did a Taylor expansion. It's in a published paper [0803.4002]. Δy_{t0} is the monopole or radial boost. It's what you call radial flow. The question is, what does radial flow belong to? Does it belong to all final-state particles, or does it belong to a subset? The message here is that it belongs to a subset which also has a quadrupole modulation. These particles are coming from a boosted source with boost Δy_{t0} which could have some variation [with radius] depending on how the source is generated [what is the structure of the moving fluid, if that is a correct model?]. What these data [p. 26] tell me is that the boost distribution is rather narrow. These data are limited [in accuracy and p_t coverage], as people have pointed out. So, I can't say for certain exactly what this [boost distribution] is. But to first order the average boost is about 0.6.

Rene: And what generates that subset?

Tom: You mean the mechanism? Well some of us have been discussing that, the possibility that it's not a hydro phenomenon. It's a quantum transition with interacting QCD fields.

Rene: Which is valid then in the outgoing channel only to a subset of the particles we measure.

Tom: Right.

Rene: And that is driven by the geometry?

Note added (Tom): Yes. The initial-state geometry (e.g. eccentricity) determines the final state.

Tom: When you have spectra like this with an edge, that's a marker, very different from the single-particle spectrum. So, you can ask where are these particles [quadrupole spectrum structure] in the single-particle spectrum? If you don't see this edge somewhere there [within current errors] the upper limit for that component is about 5%. And that corresponds to Δy_{t2} being as large as it can be, half the radial boost, so that the system doesn't "suck" [no negative boost].

Guy: Go to the spectrum with the edge again. Your edge is only in your fitting lines. If I erase all the lines and handed that to an undergraduate lab student and asked them to draw a curve through it it would not suddenly fall off to zero there.

Note added (Tom): The edge and its position are inferred from $v_2(p_t)/p_t$ data plotted vs y_t with proper mass assignments, where the data strongly favor a nonzero intercept point on y_t. Quadrupole spectra in the form $\rho_0(y_t)v_2(y_t)/p_t$ with edge are then reconstructed from the data. A boosted Lévy distribution describes the data well. That description by itself did not motivate inference of an edge.

Rene: And furthermore your edge is at 90 MeV/c p_t.

Tom: That's for pions. For protons the edge is near 1 GeV/c.

Jiangyong: Is this after subtracting the soft component?

Tom: No. The spectrum soft component by the way is up there [p. 25, left panel, dotted curves]. You can barely see them. This is just published $v_2(p_t)$ data. What's been done to those [v_2] data is shown right here [vertical axis label]. It's the measured v_2 times the measured single-particle spectrum divided by p_t.

Note added (Tom): This is not a soft/hard spectrum decomposition. This is an analysis of the quadrupole correlation component.

Denes: Is it possible if I just tweak the Lévy parameters then I don't get the peak at zero. Or what if I just throw out the Lévy and did something else? Is this somehow imposed by the particular function? That it's a general feature of this Lévy distribution which provides these parameters then you get this sudden drop.

Tom: I think the most compelling evidence [for a sharp edge or sudden drop] is the proton data [pp. 26,27, middle panel]. This is simply $v_2(p_t)/p_t$. Here are the proton data. You can try to move this [left edge going to zero] over toward the left [toward $y_t = 0$]. It's not possible.

Rene: You don't have to move it all over. The last two points already go to zero-zero. I can smoothly draw a curve through that that does not have the boost.

Tom: See this dash-dotted curve going through these data like so [based on a sharp edge]? That's a quite small error bar [protons]. Certainly the curve could be deflected to arrive at zero-zero, and then the boost distribution, narrow about 0.6, would acquire a small tail going down to zero boost. But most of the proton points imply a narrow boost distribution at 0.6. Then there are a few points that might admit another possibility. This isn't definitive [with the available data]. This is meant to illustrate a method, and to indicate that measurements at smaller p_t with heavier hadrons are most important [Tom: for testing hydro predictions of a flow field]. The data at larger p_t are irrelevant, are simply reflecting the ratio of tails of soft distributions.

Rene: I think what is most relevant is the pions at low momentum. Because the pions carry most of the tensor.

Tom: But they are the worst thing to test hydro [Tom: see detailed note added above]. You'd have to have many measurements below 0.1 GeV/c for pions to test hydro [boost distribution]. The more massive the hadron the better. $v_2(p_t)$ is the result of a folding integral [Tom: boost distribution and unboosted spectrum]. The boosted spectrum near the maximum is not determining. You are most sensitive to the boost where the slope is largest.

[several overlapping remarks]

Uli: You don't measure it by adding one velocity to another because you have thermal smearing which is larger for light than for heavy particles. Pions you have to treat like photons. They get blue shifted like the cosmic microwave radiation. You change the slope [of the spectrum]. [Tom: Again, see detailed note added above].

6. Minijets vs hydro

Tom: What emerges here is an apparent conflict between minijet abundance in the final state and what is needed in the initial state to drive hydro – the thermalized high energy density. These minijets [observed in correlations] are arguably what's supposed to be driving hydro. If we're seeing them in the final state you can't have any hydro.

Uli: I'm sorry, what is a minijet?

Tom: It's the structure you've been seeing in the colored plots [Duncan, Lanny].

Uli: What p_t?

Tom: Most probable is 1 GeV/c. That's the peaks in the $y_t \times y_t$ plots.

Mike: Even UA1 guys didn't claim that. They claimed 5 [GeV].

Note added (Tom): Inferred minijet [$\sim$ parton] spectra from UA1 extended down to 5 GeV. It was later understood that a contribution of 1-2 GeV came from the nonjet background within the jet cone. 3 to 4 GeV then applies to the parton energy associated with minijets. 1 GeV/c applies to the most-probable fragment momentum, which is then consistent with UA1 minijet measurements. The minijet terminology is also consistent with PYTHIA and HIJING and associated published papers.

Christina: The y_t shift 0.6, how much in p_t is that shift for a pion?

Tom: $p_t = 0.14\,\sinh(0.6) = 90$ MeV/c.

Christina: Then we don't have the sensitivity to test that kind of shift for pions, right?

Tom: Correct!

Thorsten: What about PHOBOS data. PHOBOS gets awfully low in p_t.

Rene: But there's no v_2, because it's such low momentum.

Note added (Tom): Apparently confirming a boosted quadrupole source with no contribution at low p_t.

Christina: The shift you have is decoupled from flow. You still can have flow because the shift is... you cannot exclude it. It doesn't contradict hydro because it's a tiny thing. [laughter]

Tom: Well, its 0.6. For a proton that's a big deal. The boost is a speed. This is a consequence of the mass ordering argument. People say "oh, there's mass ordering of v_2 at small p_t, that's consistent with hydro." Well, that's almost an empty statement.

Christina: Well with hydro we think of pions, that's the bulk. And maybe you have a few protons.

Tom: You might consider that this part [quadrupole] is a small fraction of the total, just as a possibility.

Christina: What does this boost really mean?

Thorsten: That's the flow field. [Tom: Agreed.]

Rene: If it's a small fraction of the total what are we after here. So, you're saying that there is a subset of the event that behaves that way [Tom: yes]. And then there is the bulk majority that behaves according to the language the rest of us use.

Tom: There's the possibility of three components. There's longitudinal projectile fragmentation [soft component]. There's large-angle-scattered partons all the way down to 3 GeV [Tom: fragmenting to the spectrum hard component] and you have a third thing [the quadrupole component].

Mike: I've really got a tough question here. If v_2 doesn't come from hydro, it comes from minijets, is that what you're saying?

Tom: No. They are distinct things. Minijets are what they are [Tom: conventional jets, mainly 3 GeV]. Then there is a nonjet quadrupole whose origin is...

Note added (Tom): Quadrupole momentum/centrality systematics and comparable minijet systematics are quite disjoint. That disconnect strongly argues against a thermalized bulk medium.

Mike: That's another way of saying v_2 [Tom: yes]. So, you're saying v_2 doesn't come from hydro, but the nonjet quadrupole comes from outer space. [laughter]

Rene: Why do you say it's a subset, because the quantity you're looking at is actually defined by all the particles in the event.

Tom: No.

Rene: Well, v_2 is defined by all the particles in the event. [Tom: No] Sure.

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Tom: v_2 results from *processing* all the particles in an event. But you could have 3 or 4 particles per event that carry that structure.

[many Nos]

Raimond: How big could that be?

Tom: This is actually what came up in our [earlier] exchange where you said "I have proof that the number of particles [related to v_2] is bigger than" something. I pressed you on that and I think we got to the Lee-Yang zeros. [Raimond: precisely] And I'm not quite sure how to process that. I think the data indicated that this quadrupole component is 5% or less of the final state. This explains problems that $v_2\{4\}$ has. Because if you go to more-peripheral collisions $v_2\{4\}$ crashes. It goes to zero where $v_2\{2D\}$ is following a smooth trend [the linear parametrization of $v_2\{2D\}$ data].

Rene: Yes, but the higher cumulants to some extent tell you this is a big structure, right? It's not just a few particles.

Tom: At mid centrality you have five percent of hundreds of particles. So, you have a lot to work with [$v_2\{4\}$ is nonzero]. But if you go down below $\nu = 2.5$-3, 5% gets you down to a few particles. All of a sudden you have nothing to drive $v_2\{4\}$, which goes to zero at that point. The last statement is based on Paul Sorensen's results within the past year.

Guy: But that's also where people don't expect hydro to work very well.

Tom: [That begs the question whether v_2 has anything to do with hydro.] $v_2\{2D\}$ is nonzero there. It's reliably measured.

Mike: I don't believe in hydro, but nothing you say convinces me to not believe in hydro. [laughter]

Tom: That's why you were invited Mike.

Uli: Can we get back to the puzzle you had with minijets and not enough to drive hydro?

Tom: The minijets are just the thing that is supposed to drive the whole system. That's how you get transport of energy into the transverse phase space. And they're supposed to thermalize. Now you have the energy density, the pressure, the gradients to drive hydro expansion.

Uli: I don't agree with that statement. What drives the hydrodynamic evolution is the bulk of the energy which sits with particles that have momenta less than 0.5 GeV/c. These are not minijets. What drives the hydro are the gradients of the thermal pressure which is dominated by particles with thermal momenta, not by minijets with momenta above 1.5 GeV.

Tom: Tell Kari Eskola that. This is what saturation scale arguments are supposed to do. They're supposed to provide a mechanism to get energy into the transverse phase space. And they're [minijets] supposed to thermalize.

Uli: But it's not the particles that start out with p_t larger than 1.5 GeV/c that drive the hydrodynamic evolution, because the energy contained in those particles is a small fraction of a percent of the total energy.

Note added (Tom): One should distinguish between a transverse parton energy spectrum extending down to 3 GeV and the resulting fragment momentum distribution extending down to zero p_t.

Tom: But the low-p_t hadrons that you're talking about came from 1,2,3 GeV partons, according to Kari Escola.

Rene: You also have to be consistent with what you showed us yesterday. You showed us that even if I believe in the minijet picture it's about 30% of the [total] multiplicity. So, you have 70% of the multiplicity that is "bulk."

Tom: No, your "bulk" is coming from longitudinal projectile nucleon fragmentation, as it did in p-p collisions.

Rene: But, you haven't shown that yet. I could just as well say that's a hydrodynamic bulk. Because there's no correlation structure in that 70%.

Tom: That's what Glauber linear superposition is about. If I have one p-p collision I have 2.48 hadrons per unit rapidity that come from the projectiles, soft interactions. The rest (small fraction) of 2.5 come from large-angle parton scattering. Now, if I "Glauber that up" to central Au-Au I have to keep the longitudinal fragmentation contribution. But, what is typically done is to absorb that [soft] component into [low-energy] jet production. It's swept under the rug. You can't do that a priori. You have to prove that something like that has actually happened. Without proof otherwise, 70% of central Au-Au still comes from longitudinal nucleon fragmentation (soft Pomeron exchange).

Yuri: In terms of number of particles.

Tom: Yes, that's what Glauber predicts. If you want that 70% to go somewhere else [hydro phenomena] you have to provide a mechanism. You can't just say oh that's "bulk."

Rene: You can't just say it comes from longitudinal fragmentation, because there's no correlation structure that goes with it.

Tom: No, I say that because that's what happens in p-p collisions. Then I say *unless someone proves otherwise* it still happens in Au-Au.

Rene: But then you have radial expansion.

Uli: The fact that you have radial flow and elliptic flow tells you that these guys have started to talk to each other.

Tom: "The fact that you have radial flow" comes from what evidence?

Uli: You saw it all day. Come on.

Tom: Not "come on." What evidence is there for radial flow?

[overlapping conversations]

Tom: Glauber linear superposition is a reference. Clearly, Au-Au collisions depart from that for more-central collisions. So, what we're talking about is differentially what is the departure from that reference.

Denes: That's what Uli says. He has a nice little reference called hydro where he can understand this difference. He can study system size dependence, centrality dependence, p_t dependence.

Tom: Hydro provides a *sufficient* description in certain cases over limited kinematic domains. But, is it *necessary*? Can we describe everything in the collision with something else which we should expect a priori?

Uli: That is a very valid question. I think the answer so far is no. I have not seen anything so far that gets anywhere close.

Rene: Can you for example describe the mass dependence of the shapes of the spectra? without [any reference to] radial expansion.

Tom: Let's look at protons vs pions [sketches at the board]. What we plot is the hard component in central Au-Au vs p_t. What we see is for central collisions in Au-Au [plots with suppression at large p_t/y_t and enhancement at small p_t/y_t]. R_{AA} does something like that [only suppression at large p_t]. But if you just plot the hard components you see something like this [discussed in detail in 0710.4504]. That's for pions. If you do the same thing for protons you see something like this [peak at 1 GeV/c with slight increase on high side in more-central collisions].

Uli: What is that thick solid line that falls in the pion...?

Thorsten: In-medium fragmentation function [fragment distribution] divided by the vacuum fragmentation function [fragment distribution].

Rene: This is not data, it's a model.

Tom: This is data. This is public. Read the paper [0710.4504]. This is r_{AA} ratio of hard components [data/reference] and this is the hard component. A perturbative calculation with Borghini-Wiedemann fragmentation modification follows this [the data]. This is where the baryon anomaly comes in. There is something here [in the proton hard component] which is very much like in pions except that it's displaced and then killed [FF modification stops above 1 GeV/c for protons]. Is this because the proton mass is about 1 GeV/c^2 or something else?

Rene: The basic question was, forget about soft and hard, you get a pion spectrum and a proton spectrum in heavy ion collisions. Can you explain the difference in the shape?

Tom: No. [Not completely, but Borghini-Wiedemann provides guidance.]

Rene: I can with radial expansion.

Tom: You can explain it over what p_t interval?

Rene: Over 99% of the particles.

Tom: That's not my question. Over what p_t interval?

Mike: You sound like me now. [laughter]

Rene: From 0 to 2.5 GeV/c, where 99% of the particles are.

Tom: What I care about is this. You see this centrality dependence here [10 GeV/c]? It exactly matches this centrality dependence here [0.5 GeV/c]. What's happening at 10 is the same as what is happening at 0.5. Similarly for protons.

Rene: So, what happens to one particle [at high p_t] is the same as what happens to a thousand particles at low p_t and you say I can make a 1-to-1 correspondence?

Tom: How is hydro, which is presumably a very low-p_t phenomenon, perfectly matching a trend at 10 GeV/c?

Rene: It doesn't, hopefully.

Tom: It does. Another piece of the problem: Here is published $\langle\beta_t\rangle$. We take the published $\langle\beta_t\rangle$ values and we plot them against ν, and here's what we find. Here is the p-p value at 0.25. [The data fall on two linear trends with different slopes.] Here is a break in the linear trends.

Rene: I can also say that the expansion velocity staturates.

Tom: Where does that break come?

Rene: There is no break in the line. It's smooth saturation. I can draw it without the break in the line.

Tom: It comes at exactly the sharp transition in the minijet peak. That's because what the blast-wave fit [from which radial flow is inferred] is doing is accommodating the hard component that we know is jet correlated.

Thorsten: You can turn it around. Somehow what you identify as the modification of the jet is a flow-driven phenomenon.

Tom: I don't want to necessarily convince you of this. But it would be nice if we are aware of these possibilities, and that we need to be studying this stuff in different plotting formats. Our picture is not correct until we understand the same thing in every plotting format. We see that some formats are better than others. Some kinematic domains are more critical to testing theory than others. $v_2(p_t)$ on p_t is to my mind the least able to test hydro of any plotting format.

7. Is hydro falsifiable?—Comparisons with theory

Denes: Do you call something else hydro than what Uli and Edward would call hydro?

Tom: I'm only talking about a boost distribution. That's all I know. That's what I think the hydro people should be calculating: a velocity distribution for a medium out of which comes particles. We measure the particles and we can then invert those data [to infer the boost distribution].

Denes: There could be [various quantities distributed].

Tom: Yes. This is a cartoon of what should really be done. I assumed a fixed temperature for a source, and then...

Denes: [comments on spatial dependence and other complications in hydro calculations]

Tom: I stipulate this [hydro] is a complex problem. I'm an experimentalist with some data.

Uli: The only reason I would entertain constructing models that are hard to justify on a broader basis is if I saw clear evidence that the hydrodynamic model breaks down in an essential part. I have not seen that. You have not presented me with anything that would convince me that I have a serious problem with hydrodynamics. You have pointed at some details and corners of phase space which involve a few particles. But the bulk of the picture is solid. So, why should I worry and start inventing new methods before you have killed the key [elements of hydro], really have killed the model. This is not how science proceeds. You don't invent, you don't explore the infinity of possible models just because there is some little detail in the description not quite working out until you are convinced that this detail is a make-or-break detail of the model. That case needs to be made here. Otherwise I'm just wasting my time.

Lanny: So Uli, what is an essential aspect?

Uli: You tell me what you see is crucially wrong. I don't buy that this [present discussion items] is crucially wrong because that is...when you shift stuff around in a way that I don't quite understand which suddently opens up a space in the lower left corner where basically no particles ever sit.

Tom: Why don't they sit there?

Uli: Because there are none.

Tom: Well Romatschke says they ought to be there.

Uli: OK, turn this curve...This is where you have a problem with Romatschke, right? [below $y_t = 1$].

Tom: [Yes] And for proton data you see it is a big problem.

Uli: OK, tell me how many protons out of the 50 or so you get per event sit in this position.

Note added (Tom): That interval is actually near the peak of the proton spectrum, wherein fall a large fraction of all protons.

Tom: I don't care how many there are.

Uli: If it's half a proton I say so what.

Derek: I had another question. This Romatschke thing. You drew this curve with some pretty thick lines. And you did that for a reason because you estimated the size of the numerical error. When Romatschke drew this plot, he drew his curves with very thick lines. He drew it this way because he estimated the size of his numerical error. The size of the lines which he drew is comparable to the region under study.

Note added (Tom): The line widths are not intended to estimate a numerical error (uncertainty).

Tom: OK, these lines, if you make them thick enough the theory is not falsifiable.

Rainer: There should certainly be some uncertainty bands around the curves as well. And chemistry is also a part of hydro.

[many overlapping conversations]

Tom: The point of this was not to say that hydro is dead. The point is to say that we can test hydro in ways other than [and better than] conventional methods. I issue this as a challenge to experimentalists and theorists, that this [$\rho_0(p_t)v_2(p_t)/p_t$ vs y_t with proper mass] is the interface where people should be talking to one another about hydro, not v_2 vs p_t. That's the worst interface.

Christina: You are focussing on extreme cases. If you would say it's maybe 50% of all the particles we use it would be a more convincing case, instead of really really high p_t and really really low p_t where we even cannot measure it with the detector. You have to go a little bit more to the middle.

Note added (Tom): The points made involve consideration of almost all particles in the final state, only some of which may actually participate in phenomena presently interpreted in a hydro context.

Tom: This region [y_t below 1] is the maximum of the [single-particle] distribution.

Rene: No. You have to show this is different for each particle, first of all. But then you also have to show the region of applicability of hydro on this plot, which is from zero to 2 GeV/c in p_t. So, now you translate for every particle into the y_t space.

Note added (Tom): The region of applicability of hydro should be determined by critical tests of the theory, not by a priori assumptions. It is possible that hydro is applicable nowhere.

Tom: 2 GeV/c is this ($y_t = 1.5$) for protons and this ($y_t = 3.3$) for pions. And that's where most of the particles are.

Rene: OK. So, the Romatschke curve is doing well except for the lowest points [for pions]. Most of the pions are between two and three.

Note added (Tom): Most of the pions are *below* $y_t = 2$, which is $p_t = 0.5$ GeV/c.

Thorsten: But he wants to argue protons. We had this before.

Tom: That [$y_t = 1$] is 0.15 GeV/c for pions, right at the edge of the p_t acceptance.

Rene: Let's bring out your argument. The Romatschke curve does really well for pions. You would agree with that, except for the lowest point. It doesn't do well but it's a 90 [Tom: 160 actually] MeV/c point.

Tom: My point is these pion data are not good enough [low enough in momentum] to test anything.

Rene: OK, so you're saying they don't reach far enough down so you want to look at the protons.

Tom: Right.

Thorsten: OK, can we bring up the proton point again.

Tom: You see what's most important is right here [p. 26, middle panel, proton data point to zero intercept at 0.6, not the origin]. Uli says we can squeeze by and go this way [toward the origin]. So, we need an assault on the lowest p_t for the heaviest mass hadrons. Protons is a nice tradeoff [abundance vs mass]. So, this is where you test hydro. You don't test hydro at the maximum [of the distribution]. You are asking for the boost distribution in the folding integral. If you move a distribution back and forth near its maximum you get no change. You need to go where the slope is largest.

Rene: That's your first point, go back to the v_2 measurements, that's the Lambdas there, which are comparable to the protons. So, the first two points on this, where the v_2 is tiny. It's 3% or something at most.

Note added (Tom): The values of v_2 are small, but are very significant compared to the data uncertainties, since that is where most of the particles reside which carry v_2.

Tom: These are the published errors [small compared to the data values].

Christina: This y_t looks very large on your scale but it's 1% of all the particles. You give these pions so much credit that they can test the whole hydro.

Note added (Tom): A substantial fraction of final-state pions (30%) is located below 0.2 GeV/c in p-p collisions for instance. The fraction increases in more-central Au-Au collisions.

Tom: I said these pion data can't test anything [about hydro].

Christina: So then the protons, which are also only a few percent...it's kind of the question why they should test the whole hydro picture when all the 90% are...

Lanny: I was just trying to do this conversion. The first six [v_2] points on the left in the middle panel (up to 2 GeV/c) represent almost all the protons. [Tom: Yes.] That's more than 90% of the protons.

Rene: And what we're trying to say is we can draw a curve through those that goes to zero-zero [the origin].

Uli: Romatschke didn't quite get the radial flow right. His values for the average p_T are too high for pions and kaons.

Rene: He didn't get the slope quite right.

Uli: He didn't do a complete fit of the data, did he?

Tom: He didn't do a fit of the data?

Uli: He didn't do a complete description of the single-particle data for central collisions. You have to understand that hydrodynamics is not a parameter-free theory.

Tom: Yes, I know that. [laughter]

Uli: So, you have to do a few things. You have to find the initial conditions, start the hydro and so on. And that you have to pin down. I don't think Romatschke has done a complete job of that.

Derek: I can say a few things about that. When we looked into pion scattering on nucleons in RQMD, we found that we still have a lot of pion scattering on nucleons below a temperature of 160 MeV. The pion wind pushes this (the proton v_2 at small momentum) out.

Tom: By hypothesis.

Derek: Which is a pretty well experimentally founded [hypothesis].

8. Minijet survival to the final state vs hydro

Tom: OK, how do these minijets come out [survive], because you...

Derek: Nothing you see in the hadronic phase is partons. [obscured by conversations, something about pion-nucleon rescattering] The pion wind pushes this out.

Tom: By hypothesis.

Uli: No, not by hypothesis, by well-known nucleon-pion interactions.

Derek: You put in the Delta resonance cross sections and (find that) nucleons scatter six times. It shifts those nucleons out from zero p_t, which is what you're talking about, to just a little higher in p_t . And that causes this curvature [in v_2] at low p_t; this is exactly what you are talking about. I think that's what you're seeing there. And that's why the effect [in v_2] is not there [in Romatschke].

Tom: One problem with these medium effects is that if you have something which can kick a proton around like that why are we seeing the minijet correlations? These are fragile little pions.

Rene: We could turn this around a little bit and say is there any irrefutable evidence for minijets in central Au-Au collisions? anything that I couldn't explain with something else that people have already published? For example the soft ridge, initial conditions plus....

Tom: Lanny gave a very long list at the end of his talk.

Rene: I don't think so. I think he gave a long list of things that were measured. The question is can they be explained only with minijets. Is there anything that...

Lanny: So, you have the same-side ridge that's elongated. You have the away-side ridge that follows the first in magnitude. You have charge ordering in η, ϕ and on y_t. The last is particularly relevant.

Jiangyong: But this minijet doesn't look like the minijet you see in p-p right? Of course there are similarities but also very different features. So there may be other object which is coupled with medium.

Lanny: It goes without saying that a pQCD minijet as in HIJING doesn't fit the data.

Rene: So why do we continue to call it minijets. I mean, it's not a minijet. It's much more likely either an initial condition imprinted or even a medium response. Because you actually show that it doesn't behave like minijets when you go through your transition point.

Lanny: We have about ten theoretical efforts. There are quite a few *attempts* to explain bits and pieces of all these sets of data. I'm not aware of any one that gets it all. Something disturbs the initial condition. There are some claims that radial flow explains the same-side ridge. But I don't believe there's anything on the away side [in such models], other than a global momentum conservation that's going to be way too small.

Thorsten: Why? Why should this be global momentum conservation?

Lanny: Well, you have global momentum conservation whether you like it or not.

Thorsten: But why should this be distributed across all the particles on the away side?

Lanny: It doesn't have to be. There could be some mechanism. I'm asking to see that [mechanism presented clearly], if someone claims something else could explain it [minijets]...

Rene: Can you explain the phenomena you see with a minijet picture? I would say you can't.

Lanny: I have no explanation. [For η elongation.]

Rene: Then let's not call it minijets. I think that's really confusing.

Lanny: I will remind you that in p_t correlations we still see a minijet-like structure in the momentum.

Uli: OK, so you have these correlations. If I count up the energy of all the particles that you have in these correlations what fraction of the total energy is that?

Lanny: That's a good question.

Uli: That's a permil or less.

Rene: No, if it's 30% of the particles it's not going to be a permil or less.

Uli: What is 30%

Rene: That was in Tom's presentation.

Tom: It's [minijets, same-side jet peak] 30% of the particles and the mean p_t is 0.5-0.7 GeV/c.

Note added (Tom): The soft component comprises 70% of the final state in central Au-Au collisions with $\langle p_t \rangle \approx 0.35$ GeV/c. The hard component, corresponding to minijet angular correlations (2D same-side peak), has $\langle p_t \rangle \approx 0.5$-0.7 GeV/c in central collisions (down from 1.2 GeV/c in p-p due to FF modifications). Thus, minijets carry about 40% of the transverse momentum in the final state, none of which may be thermalized. The other 60% is then carried by projectile-nucleon fragments.

Lanny: So, it would be more energetic.

Rudy: What is more difficult for me to understand is the transparency. The away-side jet.

Thorsten: Because it's not a jet. It doesn't look like a jet. We just agreed we don't want to call it jets anymore.

Yuri: Let's try to forget this confusing word minijet. Let's just address this bunch of trustable experimenters who claim they are seeing strong back-side correlations between particles of 1-1.5 GeV/c. Full stop. Don't give it any other names. Be it jets or whatever. Just the matter of fact. My question is: does it pose a problem, at least at the qualitative level, for people who are getting strong medium interactions etc. etc.? What about the transparency? It doesn't matter whether there are many charged particles or a small fraction. It's just a probe. It's just two guys, and the second survives.

Thorsten: Hydro is capable to give you these kinds of correlations. The Brazilian group got these out.

Note added (Tom): Not with the observed p_t.

Lanny: Yes, but we already discussed that and nobody [has explained how their model produces the away-side correlations]. I have not been able to understand how they get an away-side correlation. Do you understand that?

Rene: You have local momentum conservation. If you have it on the same side you have to have it on the away side.

Thorsten: Imagine something like a shock wave traveling through because you have a hot object here, radial flow pushes it out, it pushes back against flow, shock wave travels through the medium, hits the other side, freezes out, spray of particles comes out. Bang, there's your answer.

Dylan: Should there be a wider spray of particles on the away side?

Rene: And there are.

Thorsten: There's coming flow in...

Tom: There's not much different [azimuth] widths [in central Au-Au] than in p-p.

Thorsten: It's focussed the same way as the near side is because of the same flow argument.

Lanny: There's one thing I didn't mention this morning that you also have to keep in mind.

Thorsten: Uli asked qualitatively if there is a mechanism, and there is.

Rene: People didn't look enough at the away side I admit that. But there are theoretical mechanisms to do that.

Lanny: Let me remind you of one thing. Clearly, whatever is going on, there will be momentum on the away side. That momentum obviously will still be there somehow. What this plot shows you—this is away-side $y_t \times y_t$— as you go to more-central collisions you can still have the away-side momentum, but if it's going through a shock wave or if it's sound propagation or something coming out on the other side it's got to be dispersed among a lot of different particles. This [plot] is telling me you still have *pairs* of particles...you have a 1 GeV/c particle here and you're still getting a 1 GeV/c particle on the other side. If it [parton energy] was dispersed into sound I would have 1 GeV of momentum here, one there, but scattered amongst half a dozen particles. So, on $y_t \times y_t$ the peak should move down here and there [to small y_t]. This [the away-side peak] is going to [should for dispersed momentum] dissipate and melt down. That doesn't happen. So, you have to remember, the momentum is there, but it's still held by just a few particles.

Thorsten: It's still among a few particles, yes.

Yuri: And this looks strangest to me for hydro explanation.

Lanny: So it does not thermalize really, the energy [is dispersed].

Rene: It definitely stays focused spatially. That's for sure. The question is whether in terms of the ridge that you see on the away side does the structure make sense? You're saying [asking] does the p_t distribution in that structure make sense, after you go through the medium?

Lanny: That's right. It's not just the shape, but also the p_t structure.

Christina: Lanny, you say this is 30% of the particles?

Tom: The same-side peak corresponds to 30% of the particles.

Christina: Then that's the 30% highest-momentum particles. Then your mean p_t is smaller than 1 GeV/c. Maybe the counting isn't...

Note added (Tom): The p_t of particles included in the same-side jet peak extends down to 0.3 GeV/c in p-p collisions (most-probable $p_t \approx 1$ GeV/c) and below that in more-central Au-Au collisions (most-probable $p_t \approx 0.5$ GeV/c).

Rene: You also have to find those 30% on the other side. You could argue they scatter outside the acceptance, but somehow your integrals have to match up. If they're not matching up in number they have to match up in energy.

Tom: The 30% is particles that fall inside the acceptance. They're measured and they belong to the same-side peak.

Rene: So, you could ask how much you have on the away side in terms of energy and number and then you could make an argument that you're not catching all of them because it's scattered.

Tom: The away side includes only [pairs of] detected jets. So, for a given jet in the acceptance in central Au-Au about 1/3 of the time the [jet] partner also appears in the acceptance. The other 2/3 appear outside.

Rene: If you know that, it should be exactly one third.

Note added (Tom): Only a fraction of jet momentum appearing within the same-side *intrajet* peak is balanced by a jet partner appearing within the acceptance. The fraction of jet partners in NSD p-p appearing within one unit of eta is 10% for example. The concept of a balancing "away-side jet" appearing on azimuth near π radians is fallacious. The away-side peak represents jet *pairs*.

Tom: No, that doesn't matter. What matters is only the same-side peak. All jets appear in the same-side peak.

Lanny: Right, that's a thing to keep in mind. All of them, whether they're single jets or back-to-back pairs all integrate into the same-side peak. The naming [away-side jet] is horrible. It misleads people all the time.

Tom: There is no “away-side jet.” That’s a fallacy.

Rene: There’s got to be momentum conservation.

Note added (Tom): Yes, within 4π steradians, not within a restricted η acceptance. The main reason is the initial-state x distributions of scattered partons.

Helen: Yes, but Rene, how they do it, it all appears in one place. There isn’t an “away side” in their math. Because the away-side and near-side are collapsed into the same phase space.

Note added (Tom): There is no choice in the matter. All accepted jets appear in the same-side peak.

Rene: I understand that, but then the momentum conservation argument you’re making against....

Tom: If there is a singleton jet, meaning its partner is outside the η acceptance, the singleton is not momentum matched. There is [then] no momentum conservation *within the acceptance*. Momentum is conserved in 4π only.

9. Is the peak on $\mathbf{y_t \times y_t}$ created by the choice of variable?

Derek: When you plot this y_t it’s a funny transformation. Is it [the peak] so dramatic, or is it just everybody [all of the particles]? How does it change if you change this arbitrary scaling mass, which could have been anything from m_π to m_{proton}?

Tom: Not in these plots [p. 26]. In these plots there is the proper mass for each hadron.

Derek: $y_t \times y_t$ is unidentified [hadrons].

Tom: Yes, there it is the pion mass.

Derek: So, how does it change if you change the arbitrary number [mass] from Λ_{QCD} to 5 GeV?

Tom: You don’t want to do that because the y_t is there because of the underlying parton p_t spectrum. What matters there is the p_t of the parton.

Derek: You chose the number there, m_π. You could have chosen anything.

Rene: Well, the argument he’s making there is that the proton/pion ratio at your maximum y_t is larger than 1. So, you’re taking the wrong mass, in order to do your y_t scale.

Tom: I don’t know how that follows.

Rene: Well, because you divide by the pion mass to get the y_t.

Tom: It’s only to make a logarithmic momentum scale. It could have been half the pion mass, or a tenth of the pion mass. It’s just to regularize that [logarithmic] variable. Just think of those axes as log of p_t, that’s it. Changing the mass will only shift the plot sideways along the axes. That’s a mathematical fact.

Derek: Change the mass by a factor five.

Tom: Then you will shift the plot by the natural log of 5.

Rene: But half your particles are protons, half pions, so they’re not shifting to the same value in p_t.

Tom: If everything is changed by the same factor it’s only a translation on that variable [same for all hadron species]. If you use identified particles then you’re addressing a different problem. The peak is there [on y_t or $y_t \times y_t$] because of the end [cutoff] of the parton [p_t] spectrum.

Derek: You have $p_t + m$ in your y_t.

Tom: $p_t + m_t$. That's the analog in transverse space...[of $E + p_z$ in longitudinal space].

Derek: So, it's in the numerator and denominator...

Tom: No. The denominator is the mass of the hadron. In the numerator is the transverse mass m_t. I'm not sure what you're getting at.

Derek: I'm saying it's [m_0] in the numerator and denominator. It's not just a shift.

Yuri: Since we have the good fortune to return to this question of kinematics, I have a more philosophical question related. I don't understand how on earth the pion mass can enter the game at all. You're telling us that is 99% of the tensor, but that's on the late times, when everything is finished. When you are in the medium could you just tell me in MeVs what is the product of the pion cross section multiplied by the density of scattering centers, $\sigma\rho$?

Derek: When the hydro elliptic flow is developing, our mean free path that we are using is like...scales with the temperature, so is about 1/3 of the temperature.

Yuri: So, this is smallish thing, right? So, if the cross section is strong if I take $\sigma\rho$ it will be few hundred MeV. Then, pion mass doesn't matter, because when one pion propagates and interacts so many times you have extra phase. So, the effective mass is irrelevant.

Derek: I'm not worried about the pion mass. I have a problem saying what he's saying survives as the correlation function. In there was a definition of a variable which had an arbitrary number, which I'm worried about shifts this plot to some obscure place in phase space. And it's that peak in phase space that we're seeing which is an artifact. That's what I'm worried about, is whether the [jet] correlations really do survive, where in phase space they are. Because we should be able to in zeroth order understand them.

Yuri: This variable which Tom has introduced makes sense only if you want to compare particles with different masses. Otherwise, it is just a trivial algebraic shift which wouldn't change anything.

Derek: But it's $p_t + m_t$. That m_t is unknown. And then you put there some pion mass over m_t. And so it's not just a shift. So, I'm worried about could it squeeze the corner of a relatively broad distribution in phase space into a corner. That's what I'm worried about.

Yuri: It may affect the shape at small p_t when it becomes comparable to m.

Derek: In that range we know that the baryon to pion ratio is one. And so, if you take the pion mass or the proton mass there it makes a big difference, right?

Yuri: Sure.

Tom: David has plotted for you y_t with pion mass vs $\log(p_t)$. It's a straight line down to about 0.2 GeV/c. The mass then operates only as a shift. I know it appears in m_t, but that only matters down around 0.2 GeV/c and below. So, it's completely irrelevant to the *observed* structure on $y_t \times y_t$. The purpose is to get a logarithmic variable that goes to zero gracefully. That's it. On the other hand, in this case [p. 26] the mass is very important because now we want a true velocity measure in order to infer boosts of hadron sources. Two different cases – they should be kept distinct.

Christina: But why not have a traditional plot on p_t? the $y_t \times y_t$.

Tom: Actually, that's the way we started. The problem with that... We started out on $m_t \times m_t$ [in 1998!], but then all the interesting stuff [jet structure] was stuck in one bin in the upper-right corner. That's why we went to a logarithmic variable.

Derek: That's what I'm worried about. All the interesting stuff is in one bin.

Note added (Tom): The Jacobian from y_t to p_t is $dy_t/dp_t = 1/m_t$, which cannot produce a peaked structure (non-monotonic slope) from a non-peaked distribution (monotonic slope). The Jacobian from y_t to $\ln(p_t)$ is $dy_t/d\ln(p_t) = p_t/m_t$, which is approximately 1 for p_t above mass m_0 and p_t/m_0 below m_0. Again, no peaked structure can be created from non-peaked by transformation to y_t.